
letter to the editor

COMMENTS ON "INFANT MORTALITY IN NEWARK, NEW JERSEY"

The authors of this article [*Public Health Reports*, Vol. 93, July-August 1979, pp. 349-356] have presented a very interesting, but not very enlightening, discussion of the sociodemographic and medical factors associated with infant mortality in Newark, New Jersey. The conclusion that better care shortly before, during, and after delivery would virtually equalize nonwhite and white infant mortality rates (p. 355) is in no way substantiated by the data presented. The epidemiological information obtained has been interpreted as causation rather than association.

The ease with which a number of variables were dismissed as having little or no influence on birth weight and infant mortality was quite remarkable. For example, it is noted on page 355 that if improvements in housing or nutrition, factors that may affect infant mortality rates, were responsible for the decrease in the infant mortality rate observed between 1970 and 1973, then they would be likely reflected in a decreased frequency of low birth weight babies. Since a decrease in incidence of low birth weight babies was not observed, the authors imply that these were not important variables. How can that conclusion possibly be made when the authors did not know

what had happened to housing or nutritional status during that period? It seems that an equally appropriate interpretation would be that the incidence of low birth weight infants did not improve because there was not an improvement in housing or nutritional status. Because of the thoroughly documented effect of pre-pregnancy weight status and weight gain during pregnancy on infant birth weight (1-4), control of these variables must be obtained in any study which attempts to identify the causes of low birth weight among any given target group.

The authors present no data whatsoever on pre-pregnancy weight nor weight gain during pregnancy, but somehow conclude that nutrition was probably not related to the decline in the incidence of low birth weight by stating that protein-calorie supplementation has not been documented to increase birth weight in the United States. The study cited to support this statement refers to the work performed by Rush et al. in Harlem (5,6). The authors state on page 355, "indeed, under controlled conditions, high protein, high calorie supplements during pregnancy has not been associated with beneficial effects." Because of the methodological problems associated with the Harlem study, such a conclusion is not warranted. The basic methodological flaws that make the results of this study extremely difficult

to interpret are that: 1) total dietary calorie and protein intake was not determined, so that improvements in protein and calorie intake resulting from the supplement could not be assessed; 2) the nutritional status of the women prior to pregnancy is not documented, and 3) the availability and utilization of a protein mixture in a supplement beverage were not tested.

A number of studies performed in the United States clearly indicate that food supplementation, diet quality, and nutritional status during pregnancy influence birth weight (7-10). To date, no study has been carried out with the rigorous design and control needed to separate the influence of nutrition from other sociodemographic and health variables. Obviously, well-controlled studies on the effects of nutrition on birth weight and mortality are needed.

Most disappointingly, the major conclusion reached by the authors ignores the impact of preventative services on the outcome of pregnancy. As demonstrated by the Swedish, Dutch, and Finnish experiences, low birth weight to a large extent is preventable (11). It would seem that equalization in nonwhite and white infant mortality rates could not be achieved until the incidence of low birth weight is also equalized. As Stein, Susser, and Rush point out (12), an ostensible key to

improving perinatal survival is to try to raise birth weight. Acute intervention around the time of delivery will have no impact on birth weight, but rather represents the classic, costly role of health care in putting out fires rather than preventing the flames.

References

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IN REPLY

Drs. Brown and York first decided we implied something about housing and nutrition and then immediately escalated that to indicating we "concluded." Imply and conclude are different words with different meanings. In any case we did not intend to imply that housing and nutrition played no role, and we never suggested we studied these variables. However, if such influences were important, we would have thought this would have been expressed as a change in birth weight. Their alternate suggestion that birth weight might not have changed because housing and nutrition did not change would not alter our observations that mortality fell substantially despite no change in birth weight. Their statement that we concluded nutrition was not related to the decline in the incidence of low birth weight is perplexing since one of our major points was that mortality fell despite no demonstrable change in birth weight. Presumably they did not mean that statement, and it merely represents an oversight in proofreading their own comments.

We chose not to get into the complex debate about the efficacy of prenatal care. All we said was the "notion that routine prenatal care will reduce the rate of low birth weight (LBW) babies and thus lower infant mortality may be too facile." That is a careful and accurate statement.

At present we do not adequately understand the causes of most LBW in America or the precise role of nutritional supplements in obviating LBW. Indeed our own case control studies suggest LBW babies may have some very specific deficiencies in vitamins and trace substances (1,2). It may well be that just feeding extra proteins and/or calories will not alter birth weight rates

in the United States, as suggested by the studies of others that appear to us to be methodologically sound (3-6).

Brown and York are quite right in their comment about our statement "better care before, during, and after delivery would virtually equalize non-white and white infant mortality rates." Inadvertently in the several re-writes, we left out the modifying phrase "if the ethnic disparities in LBW can be eliminated." The rest of the paragraph focuses on the LBW issue, which as we noted is a critical issue in further reducing infant mortality. The Scandinavian studies show less prevalent LBW, but not that LBW existed in substantial proportion and was prevented by specific intervention. We believe that our study showed that even without modification of LBW prevalence the intensive care of high-risk neonates did reduce infant mortality to a striking degree.

References

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